

(now destroyed) at Chiswick. J. Platt Barrett, "The Butterflies of Sicily." Includes "sixty-five Sicilian butterflies out of the ninety-seven given by Ragusa." Dr. T. A. Chapman, "On insect teratology" (remarks to introduce discussion on teratological specimens). W. J. Kaye, "An entomological trip to South Brazil" (Lepidoptera). W. J. Lucas, "The natural order of insects Neuroptera." A general sketch of the families, with illustrations of examples. R. A. S. Priske and H. Main, "Notes on the glow-worm, *Lampyris noctiluca*." W. J. Kaye, annual address, read June 26, 1911. The address is chiefly devoted to the neurulation of Lepidoptera, but also contains obituary notices, &c., that of J. W. Tutt being specially noteworthy.

The volume also includes a list of members at the beginning, and an abstract of proceedings at the end.

TRANSMISSION OF TRYPANOSOMES.

IT will be most unwelcome news to many that, according to a recent number of the Bulletin of the Sleeping Sickness Bureau (No. 29, August 17), Dr. Taute, at Tanganyika, has succeeded in transmitting a human trypanosome to monkeys by means of *Glossina morsitans*. So long as it was believed that *G. palpalis* alone was capable of transmitting the trypanosomes of human beings, it was hoped that the range of sleeping sickness would be coterminous with the distribution of this species of fly; but if other tsetses also can transmit the disease, there seems to be no reason why it should not spread over practically the whole African continent. Too much weight must not be laid, however, on laboratory experiments, the success of which, as the editor of the bulletin remarks, does not prove that the like is of common occurrence in nature.

Quite recently, however, a disease of human beings has been found to occur in northern Rhodesia and Nyasaland in regions where only *G. morsitans*, and no *G. palpalis*, is stated to occur, caused by a trypanosome which has been named *Trypanosoma rhodesiense*, since it shows certain peculiarities distinguishing it from the typical *T. gambiense* of sleeping sickness.

In the same number of the bulletin a summary is given of further researches by Chagas on the human trypanosome of Brazil, of which an account was published in NATURE (August 4, 1910, p. 142). Chagas has found that the parasite multiplies in other tissues besides the lungs, namely, the cardiac muscle, the central nervous system, and the striated muscles more especially; he believes that in the lungs a multiplication of sexual forms takes place, and that the multiplication in the tissues is asexual. The infection caused by this trypanosome, transmitted by a bug (*Conorhinus* sp.), attacks the whole population in the districts in which it occurs, so that children probably all sicken in their first year, and either die or pass over to the chronic stage. The chronic disease shows various forms, but two more especially, those in which heart-symptoms occur and others in which nervous symptoms preponderate. The goitre frequently seen in the province of Minas Geraes is believed to be attributable to the same infection.

THE BRITISH ASSOCIATION AT PORTSMOUTH.

SECTION K.

BOTANY.

OPENING ADDRESS BY PROF. F. E. WEISS, D.Sc.,
PRESIDENT OF THE SECTION.

GREATLY as I prize the honour done me by the Council of the British Association in electing me to the office of President of the Botanical Section, my gratification has been heightened by the knowledge that the meetings of this section would be graced by the presence of the distinguished group of Continental and American botanists who have just taken part in the International Phytogeographical Excursion to the British Isles.

I am sure that I am voicing the unanimous feeling of the section in offering them a hearty welcome to our deliberations, and, in conveying to them our sense of the honour they have done us by their acceptance of the

NO. 2186, VOL. 87]

invitation of this Association, I should like to express our hope that by their participation in our proceedings they will help us to promote the advancement of botanical science, for which purpose we are met together.

In view of these special circumstances in which we forgather, it may seem inappropriate if I deal, as I shall be doing, in my Presidential Address mainly with fossil plants, with the study of which I have been for some time occupied; but I need hardly assure our visitors that, while we entertain some feelings of satisfaction at the contributions made during the past half-century towards our knowledge of extinct flora of Britain, yet, as the later sittings of this section will show, and as they have no doubt realised during their peregrinations through this country, our botanical sympathies and energies are by no means limited to this branch of botanical study. Moreover, I hope during the course of my address to point out the ecological interest which is afforded by certain aspects of Palaeobotany.

On the sure foundations laid by my revered predecessor, the late Prof. Williamson, so vast a superstructure has been erected by the active work of numerous investigators that I must limit myself in this address to exploring only certain of its recesses, and I shall consequently confine myself to some aspects of Palaeobotany which have either not been dealt with in those able expositions of the subject given to this section by previous occupants of this presidential chair, or which may be said to have passed since then into a period of mutation.

The great attractiveness of Palaeobotany, and the very general interest which has been evinced in botanical circles in the progress of recent investigations into the structure of fossil plants, are due to the light they have thrown upon the relationship and the evolution of various groups of existing plants. It was the lasting achievement of Williamson to have shown, with the active cooperation of many working-men naturalists from the Lancashire and Yorkshire coalfields, that the structure of the coal-measure plants from these districts can be studied in microscopic preparations as effectively as has been the case with recent plants since the days of Grew and Malpighi. Indeed, had Sachs lived to continue his marvellous historical account of the rise of botanical knowledge up to the year 1880 or 1890, he would undoubtedly have directed attention to the remarkable growth of our knowledge of extinct plants gained by Binney and Williamson from the plant remains in the calcareous nodules of English coal-seams, and by Renault from the siliceous pebbles of Autun. We are not likely to forget the pioneer work of these veterans, though since then investigations of similar concretions from the coal deposits of this and other countries have been undertaken by numerous workers, and have revealed further secrets from that vast store of information which lies buried at our feet.

The possibilities of impression material had indeed been practically exhausted in 1870, and further advance could only come from new methods of attacking the problems that still remained to be solved. The most striking recent instance of the insufficiency of the evidence of external features alone was Prof. Oliver's demonstration of the seed-bearing nature of certain fern-like plants, based on microscopical comparison of the structure of the cupule of *Lagenostoma*, with the fronds of *Lyginodendron*, after which discovery confirmatory evidence speedily came to hand from numerous plant impressions examined by Kidston, Zeiller, and other observers.

Undoubtedly in the hands of a less competent and farsighted observer than Williamson the new means of investigation might have proved as misleading as the old method had been in many instances. Indeed, as is well known, the recognition in the sections of Calamites and Sigillarias of the presence of secondary wood had caused Brongniart to place these plants among conifers, owing to his belief that no Vascular Cryptogams exhibited exogenous growth in thickness. It required all Williamson's eloquence and pugnacity to convert both British and French Palaeobotanists to his views, ultimately accepted with such handsome acknowledgment by Grand' Eury, one of his antagonists, in his "Géologie et Paléontologie du Bassin Houiller du Gard."

It is curious that Grand' Eury refers in his introduction

to the discovery of traces of secondary growth in *Ophioglossum*, and not to that of *Isoëtes*, a plant much more nearly related, as we now believe, to the Lepidodendraceæ, and the structure of which had been so thoroughly investigated by Hofmeister. Williamson, it is true, refers to the secondary growth in the stem of *Isoëtes* in his memoir on *Stigmaria*, but compares it with the periderm-forming cambium of that plant, and does not, therefore, recognise any agreement in the secondary growth of these two plants.

Adopting Von Mohl's interpretation of the root-bearing base of the *Isoëtes* plant as a "caudex descendens," Williamson instituted a morphological comparison between the latter and the branching *Stigmaria*, and came to the conclusion that they were homologous structures, a view which, as we heard at Sheffield, is supported by Dr. Lang on the strength of a re-examination of the anatomy of the stock of *Isoëtes*. If we do not accept Williamson's interpretation of the Stigmarian axis as a downward prolongation of caulome nature, the question remains open whether this underground structure represented a leafless modification of a normal leaf-bearing axis such as is known in the leafless rhizoms of *Neottia* and other saprophytic plants, or whether the Stigmarian axes were morphological entities of peculiar character. Grand' Eury, in comparing them with the rhizomes of *Psilotum*, accepted the former alternative, and, apart from morphological considerations, was led to this view by the fact that he had observed aerial stems arising in many instances as buds on the horizontal branches of *Stigmaria*. Confirmation of this mode of growth is still required, but it is quite conceivable that there may have been a mode of vegetative reproduction in the *Stigmariæ* analogous to that of *Ophioglossum*.¹

The alternative interpretation of the Stigmarian axes as special morphological entities has received weighty support from Scott and Bower, who consider them comparable to the rhizophores of *Selaginella*, which, as is well known, may either be root-bearers, or in certain circumstances become transformed into leafy shoots. This peculiarity has led Goebel to regard them as special members, somewhat intermediate between stems and roots. But though they might therefore be regarded as of a primitive nature, the rhizophores of the Selaginellaceæ seem such specialised structures that I incline to agree with Bower that, so far as their correspondence with *Selaginella* is concerned, the Stigmarian axes would agree most closely with the basal knot formed on the hypocotyl of *Selaginella spinulosa*. Seeing, however, that the nearest living representative of the Lepidodendraceæ is in all probability *Isoëtes*, which Bower has aptly summarised as like "a partially differentiated *Lepidostrobus* seated upon a Lepidodendroid base," we must inevitably consider the root-bearing base of *Isoëtes* as homologous with the branching axes of *Stigmaria*, whatever their morphological nature may have been, and perhaps we shall be on the safest ground if we consider them both as different expressions of the continued growth of the lower region of the plant, which appears to have been a primary feature in the morphology of both these members of the Lycopodiæ.

The somewhat considerable difference in external appearance between the homologous organs of these two plants may be considered bridged over by the somewhat reduced axes of *Stigmariopsis* and by the still more contracted base of the Mesozoic *Pleuromoya*, which, in spite of its very different fructification, we may unhesitatingly compare with *Isoëtes* so far as its root-bearing axis is concerned.

I was inclined at one time to seek an analogy for the Stigmarian axis in that interesting primitive structure, the protocorm of *Phylloglossum*, and of embryo Lycopods; but I now consider that the resemblances are largely superficial, and do not rest upon any satisfactory anatomical correspondence.

One of the features which has caused some divergence

¹ It is of interest in this connection to note that Potonié has recently put forward the suggestion that many of these vertical outgrowths from the more or less horizontal Stigmarian axes, some of which, as figured and described by Goldenberg, taper off rapidly to a point, without any trace of ramification, may be comparable with the conical "knees" of *Taxodium*, and represent woody pneumatophores so common in the Swamp Cypress and other swamp-inhabiting trees.

of opinion in the past as to the morphology of the Stigmarian axis has been the definite quincuncial arrangement and the apparent exogenous origin of the roots borne on these underground organs. Schimper, indeed, considered these two features so characteristic of foliar organs that he suggested that these so-called "appendices" might possibly be metamorphosed leaves. Not quite satisfied with this view, Renault endeavoured to establish the existence of two types of lateral organs on the Stigmarian axis, true roots with a triarch arrangement of wood and root-like leaves of monarch type. Williamson, however, clearly showed that the apparent triarch arrangement was really due to the presence at two angles of the metaxylem of the first tracheids of secondary wood, and reasserted the existence of only one type of appendicular organs, agreeing so closely, both in structure and in their orientation to the axis, on which they were borne, with the roots of *Isoëtes* that it would be impossible to deny the root nature of the Stigmarian "appendices" without applying the same treatment to the roots of *Isoëtes*.

Still, so distinguished a Palæobotanist as Solms Laubach, after a careful weighing of all the available evidence, continued to uphold Schimper's view of the foliar nature of these outgrowths, both in his "Palæophytologie" and in his memoir on *Stigmariopsis*, in which he stated that he was in complete agreement with Grand' Eury's conclusion: "Que ces organes sont indistinctement des rhizomes et que les Sigillaires n'avaient pas de racines réelles, ainsi que *Psilotum*." Indeed, in reviewing the account I gave of the occurrence of a special system of spiral tracheids in the outer cortex of the Stigmarian rootlets, Count Solms directed attention to their similarity to the transfusion tissue of Lepidodendroid leaves, and asserted that we have here a further indication of the former foliar nature of these rootlets. Personally, I still adhere to the belief, expressed at the time, that these peripheral cortical tracheids represent a special development required by a plant with an aquatic monarch root of the *Isoëtes* type and a large development of aerial evaporating surface. The fact that the lateral outgrowths from the Stigmarian axis have been generally considered to be exogenous is not a valid argument against their root nature, as the same origin is ascribed to the roots of *Phylloglossum* and to those produced on the rhizophores of *Selaginella*. Probably, indeed, as Bower points out in his masterly exposition of the "Origin of a Land Flora," in dealing with the Lycopodiæ, "the root in its inception would, like the stem of these plants, be exogenous." According to the "re-capitulation theory," indeed, the exogenous formation of the roots in the embryo of certain Lycopods, as well as of the first roots of *Isoëtes* and the first root of the Filicales, might be regarded as the retention of a more primitive character in these particular organs. The roots of *Stigmaria*, even if exogenous, might therefore merely represent a more ancestral stage. This difference between the roots of *Isoëtes* and the rootlets of *Stigmaria* may, however, be more apparent than real, for my colleague, Dr. Lang, has directed my attention to the fact that there appear to be in *Stigmaria* remnants of a small-celled tissue on the outside of what has generally been taken to be the superficial layer of the Stigmarian axis, and a careful investigation of this point inclines me to agree with him that very probably the Stigmarian rootlets were actually formed like those of *Isoëtes*, somewhat below the surface layer, which, after the emergence of the rootlets, became partially disorganized. Should this surmise prove correct, when apices of *Stigmaria* showing structure come to light, the last real difference between the rootlets of *Isoëtes* and the rootlets of *Stigmaria* will have disappeared, and the view for which Prof. Williamson so strongly contended will be finally established.

While a careful comparison of *Isoëtes* with the extinct Lycopodiaceous plants may be taken to settle finally its systematic position, the Psilotaceæ have been somewhat disturbed by such comparisons. Placed formerly without much hesitation in the phylum Lycopodiæ, certain features in their organisation, such as the dichotomy of their sporophylls and the structure of their fructification generally, have suggested affinity with that interesting group of extinct plants, the Sphenophyllales. Their actual inclusion in this group by Thomas and by Bower may

seem, perhaps, somewhat hazardous, considering the differences existing between the Psilotaceæ and Sphenophyllum; and the more cautious attitude of Seward, in setting up a separate group for these forms, seems, on the whole, more satisfactory than forcing these aberrant relatives of the Lycopods into the somewhat Procrustean bed of Sphenophyllales, which necessitates the minimising of such important differences as the dichotomous branching of the axis and the alternate arrangement of their leaves, though the latter character allows, it is true, of some bridging over. But, even adopting this more cautious attitude, the study of the Sphenophyllales has been of great help in coming to a clearer understanding of certain morphological peculiarities of the Psilotaceæ, quite apart from the flood of light which this synthetic group of Sphenophyllales has thrown upon the relationship of the Lycopodiæ to the Equisetales.

More far-reaching in its bearing on the relationships of existing plants has been the study of those interesting fern-like plants which seem to show in their vegetative organs a structure possessing both fern-like and Cycadian affinities. Full of interest as these so-called Cycadofilices were in their vegetative organisation, they were destined to rivet on themselves the attention of all botanists by the discovery of their fructifications. No chapter in the recent history of Palæobotany is more thrilling than the discovery, by the patient and thorough researches of Prof. Oliver, of the connection between Lyginodendron and the well-known palæozoic seed, Lagenostoma. With Dr. Scott as sponsor, this new and startling revelation met with ready acceptance, and, thanks to the indefatigable energies of Palæobotanists, no fossil fern seemed at one time safe from possible inclusion among the Pteridospermae.

The infectious enthusiasm with which the discovery of the seed-bearing habit of the Lyginodendrea and the Medullosoæ was greeted carried all before it, and we in England, particularly, have perhaps not looked carefully enough into the foundations upon which rested the theory that these groups form the "missing links" between the Ferns and Cycads. A criticism against the wholesale acceptance of this view has been put forward by Prof. Chodat,¹ of Geneva, that distinguished and versatile botanist whom we have on several occasions had the pleasure of welcoming among us. Couched throughout in friendly and courteous language, and full of admiration for the work of those who were concerned in the establishment of the group of Cycadofilices, now termed Pteridospermae, Prof. Chodat suggests that English Palæobotanists have not sufficiently appreciated the work of Bertrand and Corneille² on the fibro-vascular system of existing ferns, and have not revised, in the light of the researches of these French investigators, the interpretation given to the arrangement of the primary vascular tissues of Lyginodendron. In Chodat's opinion the structure of the primary groups of wood found in the stem and in the double leaf-trace of this plant is not directly comparable with the arrangement found in the petiole of existing Cycads. In the latter the bulk of the metaxylem is centripetal, while we have, in addition, a varying amount of small-celled centrifugal wood towards the outside of the protoxylem, and, though separated from it by a group of parenchymatous cells, the bundle may be conveniently described as mesarch. In Lyginodendron, and the same applies to Heterangium, the primary bundles of the stem appear at first sight to be mesarch too, but in Chodat's opinion, if I understand him correctly, the metaxylem is exclusively centrifugal in its development, but, widening out and bending inwards again, in form of the Greek letter ω , the two extremities of the metaxylem are united on the inside of the protoxylem, forming an arrangement described by Bertrand and Corneille in the case of several fern petioles under the name of "un divergent fermé."

Several details of structure, such as the type of pitting of the metaxylem elements and the separation of the protoxylem from the adaxial elements of metaxylem by parenchymatous cells, confirm Chodat in his view that the

primary bundles of Lyginodendron are not really mesarch, and that the stem of Lyginodendron is essentially Filicinean in nature. Chodat cites other characters, such as the presence of sclerised elements in the pith, and the absence of mucilage ducts, in support of his view of the purely filicinean affinities of the Lyginodendreae. The presence of secondary thickening in Lyginodendron, he regards not as indicative of Cycadian affinity, but merely as another instance of secondary growth in an extinct Cryptogam, taking up very much the position of Williamson in his earlier controversy with French botanists with regard to the secondary thickening of Calamites and Lepidodendreae. Chodat is also at variance with Kidston and Miss Benson as to the nature of the microspores borne on the fronds of Lyginodendron or Lyginopteris, as he prefers to call this plant. He certainly figures some very fern-like sporangia, attached to the fronds of Lyginodendron, but anyone who has worked with the very fragmentary and somewhat disorganised material contained in our nodules knows how difficult it is to be absolutely certain of structural continuity. Nevertheless a re-investigation of the whole question of the microsporangia of Lyginodendron seems to me clearly called for by the publication of Chodat's figures.

As regards the seed-bearing habit of Lyginodendron, Chodat adopts wholeheartedly Oliver's correlation of Lagenostoma with the fronds of Lyginodendron, but would regard the seed, apparently devoid of endosperm at the time of pollination, as a somewhat specialised macrosporic development, of more complex structure, but analogous in its nature to the seed-like organ exhibited by Lepidocarpon in another phylum of the Pteridophyta. "In any case," he concludes, "the origin and the biology of this kind of seed must have been very different from those of the seeds of the Gymnosperms."

This contention, based mainly on the tardy development of the endosperm in Lagenostoma, is the least weighty part of Chodat's criticism, for it has never been asserted that the seeds were identical with those of existing Cycads. We know that the seed-habit was adopted by various groups of Vascular Cryptogams, and it is revealed in fossil plants in various stages of evolution, so that it may be readily presented to us at a special stage of its evolution in Lyginodendron. Moreover, we must remember that in so highly organised a Gymnosperm as Pinus, the macrospore itself is not fully developed at the time of pollination. Though not suggesting this as a primitive feature in the case of the pine, we can well imagine how, by a gradual process of "anticipation," the prothallus might become established before pollination in any group of primitive seed-bearing plants. There are other more specialised rather than primitive features in the complex structure of Lagenostoma which might with much more reason be invoked, to show that the seed of Lyginodendron does not form a step in the series of forms leading to the Cycadian ovule.

But leaving this point out of consideration, Chodat brings forward some strong reasons for his conclusions that the Lyginodendreae were plants possessing stems of purely fern-like structure, increasing in thickness by means of a cambium, that their foliage was of filicinean structure, but provided with two kinds of sporangia, microsporangia similar to those of Leptosporangiatae ferns, and macrosporangia of specialised type, containing a single macrospore. This group, therefore, Chodat regards as a highly specialised group of ferns, which, he considers, shows no particular connection with the Cycads, and may have formed the end in a series of highly differentiated members of the Filicinae.

Of the Medullosoæ, on the other hand, Chodat takes a very different view. Both in the structure of their primary and secondary growth, as well as in their polystely, he sees close affinity of these forms to the Cycads, borne out by smaller secondary features, such as the presence of mucilage ducts and the simple form of pollen-chamber. Chodat considers the agreement of the Medullosoæ with the Cycadaceæ to be so close that he regards them as Protocycadæ, the fern-like habit being restricted to the position of the sporangia on the vegetative fronds. Medullosa, therefore, would be only one link in the chain connecting the Cycads with the Filicales, and a link very

¹ Chodat, R.: "Les Ptéropides des temps palæozoïques," *Archives des Sciences physiques et naturelles*, Genève, tome xxvi, 1903.

² Bertrand, C. E., and Corneille, F.: "Étude sur quelques caractéristiques de la structure des filicinales actuelles," *Travaux et mémoires de l'Université de Lille*, 1902.

near the Cycadian end of that chain. Other forms more closely connected with the Filicinaeum phylum are still to be sought.

In bringing Prof. Chodat's views to your notice, I do not wish to urge their acceptance, but his criticism seems to me sufficiently weighty to demand a careful reconsideration of the structure and affinities of the Lyginodendreae, which, whatever may be their ultimate position in our scheme of classification, will continue in the future, as they have done in the past, to command the attention of all botanists interested in the evolution of plant life.

If the wholeheartedness with which we in England received the theory of the Cycadian affinity of Lygino-dendron has laid us open to friendly criticism, I am afraid some of us may be accused of exceeding the speed-limit in our rapid acceptance of the Cycadoidean ancestry of the Angiosperms. Ever since Wieland put forth the suggestion in his elaborate monograph of the "American Fossil Cycads" that "further reduction and specialisation of parts in some such generalised type, like the bisporangiate strobilus of Cycadoidea, could result in a bisexual angiospermous flower," speculation as to the steps by which the evolution might have been brought about has been rife, and Hallier in Germany and Arber and Parkin in England have put forward definite schemes giving probable lines of descent. Arber and Parkin in their criticism and detailed suggestions connect phylogenetically with the Bennettitales, the Ranales, as primitive Angiosperms, and displace from this position the Amentales and Piperales, which were regarded by Engler as probably more closely related to the Proangiosperms. Of course, the resemblance between the amphisporangiate, or, as I should prefer to call it, the heterosporangiate "strobilus" of Cycadoidea, and the flower, say, of Magnolia is very striking, and the knowledge we have gained of the structure and organisation of the Bennettitales certainly invites the belief in a possible descent of the Angiosperms from this branch of the great Cycadian plexus; but the ease with which the flower of the Ranales can in some respects be fitted on to the "flower" of Cycadoidea raises suspicion. Critics of the Arber-Parkin hypothesis may possibly incline to the view that "truth is often stranger than fiction," and that the real descent of the Angiosperms may have been much less direct than that put forward in these recent hypotheses. The particular view of the morphology of the intraseminal scales and seed pedicles adopted by Arber and Parkin is, as they admit, not the only interpretation that can be put upon these structures, and the views on this point will probably remain as various as are those of the female cone of Pinus. Even if we regard the ovulate portion of the Cycadoidean "flower" as a gynoecium, and not as an inflorescence, we are bound to admit, as do Arber and Parkin, that it is highly modified from the pro-anthostrobilus type with a series of carpels bearing marginal ovules. Cycadoidea was evidently a highly specialised form, and may well have been the last stage in a series of extinct plants.

Arber's very sharp separation of mono- and amphisporangiate Pteridosperms does not seem to me quite justified. Amphisporangiate forms may have been preserved, or may have arisen anew in various groups of Pteridosperms or in their descendants. Heterospory, we know, originated independently in at least three of the great phyla of vascular Cryptogams, and originally, no doubt, the same strobilus contained both macro- and microsporangia, as was the case in Calamostachys Casheana, in the strobili of most Lepidodendraceae, and as is still the case in the strobili of Selaginella and in Isoëtes. Even in the existing heterosporous Filicinae, micro- and macro-spores are found on the same leaf and on the same sorus; and though in the higher Cryptogamia and the lower Phanerogamia there may have been a tendency to an iso-sporangiate condition, yet, as the two kinds of spores are obviously homologous in origin, nothing is more natural than an occasional reversion to a heterosporangiate fructification. Thus, in the group of Gymnosperms, we have many instances of the occurrence of so-called androgynous cones. In 1891, at the meeting of the British Association at Leeds, I described such amphisporangiate cones which occurred regularly on a *Pinus Thunbergii* in the Royal Gardens of Kew, and only this spring I was able to gather

several hermaphrodite cones of *Larix europaea*. They have, of course, been observed and described by many authors for a variety of Gymnosperms. What more likely than that many extinct Gymnosperms may have developed heterosporangiate fructifications? It is not necessary, therefore, to fix on one group of ancestors for the origin of all existing Angiosperms. Indeed, the great variety of forms, both of vegetative and reproductive organs, which we meet with in the Angiosperms, not only to-day, but even in the Cretaceous period, in which they first made their appearance, warrants, I think, the belief in a polyphyletic origin of this highest order of plants. It is no doubt true, as Wieland points out, "that the plexus to which Cycadoidea belonged, as is the case in every highly organised plant type, presented members of infinite variety," and, indeed, so far as the vegetative organisation goes, we know already, through the labours of Nathorst, of such a remarkable form as *Wielandiella angustifolia*, while Wieland has shown us a further type in his Mexican Williamsonia. Nevertheless, these diverse forms all agree in the structure of their gynoecium, the particular organ which is not so easy to bring into line with that of the Angiosperms.

I am quite alive to, though somewhat sceptical of, the possibility of a direct descent of the Ranales from the Cycadoidea, but my hesitation in accepting Arber and Parkin's view of the ancestry of the Angiosperms is enhanced by the consideration that it seems almost more difficult to derive some of the apparently primitive Angiosperms from the Ranales, than the latter from Cycadoidea. Indeed, this common origin of Angiosperms from the Ranalian plexus will, I feel sure, prove the stumbling-block to any general acceptance of the Arber-Parkin theory. It is easy enough to assume that all Angiosperms with the unisexual flowers have been derived by degeneration or specialisation from forms with hermaphrodite flowers of the primitive Ranalian type, but unfortunately some of these degenerate forms possess certain characters which appear to me to be undoubtedly primitive.

It is difficult for those who accept Bower's view of the gradual sterilisation of sporogenous tissue not to regard the many-celled archesporium in the ovules of Casuarina and of the Amentales as a primitive character, and though, as Coulter and Chamberlain point out, this feature is manifested by several members of the Ranunculaceæ and Rosaceæ, as well as by a few isolated Gamopetalæ, its very widespread occurrence in the Amentales seems to indicate its more general retention in this group of plants, and does not agree readily with the theory that these unisexual orders are highly specialised plants, with much-reduced flowers. The possession of a multicellular archesporium is, however, not the only primitive character exhibited by some of the unisexual orders of the Archichlamydeæ. Miss Kershaw¹ has shown, in her investigation of the structure and development of the ovule of Myrica, that in this genus, which possesses a single erect ovule, the integument is entirely free from the nucellus, and is provided with well-developed vascular bundles, in both of which features it resembles very closely the palaeozoic seed Trigonocarpus. The same features were shown, moreover, by Dr. Benson² and Miss Welsford to occur in the ovules of *Juglans regia*, and in a few allied genera, such as Morus and Urtica. Also in a large number of Amentales with anatropous ovules (Quercus, Corylus, Castanea, &c.), Miss Kershaw has demonstrated the occurrence of a well-developed integumentary vascular supply. No doubt a further search may reveal the occurrence of this feature in some other dicotyledonous ovules, but in the meantime it seems difficult to believe that such a primitive vascular system, which the Amentales share with the older Gymnosperms, would have been retained in the catkin-bearing group, if it had undergone far-reaching floral differentiation, while it had disappeared from the plants which in other respects remained primitive. It would be still more difficult to imagine that it had arisen in the Amentales subsequently to their specialisation.

There are other structural characters and general morphological considerations, which I have not time to deal with, which underlie the belief in the primitiveness of the

¹ *Annals of Botany*, vol. xxiii., 1909.

² *Ibid.*

Amentales and some allied cohorts, and I trust they will be set forth in detail by a better systematist than I can claim to be. My object in bringing the matter forward at all is to point out some of the difficulties which prevent me from accepting a monophyletic origin of the Dicotyledons through the Ranalian plexus.

One of these difficulties lies in the relationship of the Gnetales to the Dicotyledons. Arber and Parkin have recently made the attempt to gain a clearer insight into the affinities of this somewhat puzzling group by applying to it the "strobilus theory¹ of Angiospermous descent." The peculiar structure of the flowers of Welwitschia lends itself particularly well to a comparison with those of Cycadoidea, and a good case can no doubt be made out for a hemiangiospermous ancestry of this member of the Gnetaeæ, and by reduction the other members, in many respects simpler, might be derived from a similar ancestor, though probably, so far as Ephedra and Gnetum are concerned, an equally good, if not better, comparison might be made with Cordaites. But even supposing we admit the possibility of a derivation of the Gnetales from an amphisporangiate Pteridosperm, I think the Amentales merit quite as much as the Gnetales to be considered as having taken their origin separately from the Hemiangiospermeæ, and not from the Ranalian plexus. I find this view has been put forward also by Lignier² in his attempt to reconstruct the phylogenetic history of the Angiosperms, and I feel strongly that such a polyphyletic descent, whether from the more specialised anthostrobilate Pteridospermeæ or from several groups of a more primitive Cycado-Cordaitean plexus, is more in accordance with the early differentiation of the Cretaceous Angiosperms, and with the essential differences existing now in the orders grouped together as Archichlamydeæ.

Attempts at reconstructing the phylogeny of the Angiosperms are bound to be at the present time largely speculative, but we may possibly be on the threshold of the discovery of more certain records of the past history of the higher Spermaphyta, since Dr. Marie Stopes has commenced to publish her investigations of the Cretaceous fossil plants collected in Japan, and Prof. Jeffrey has been fortunate enough to discover cretaceous plant-remains showing structure in America. The former have already provided us with details of an interesting Angiospermic flower, and if the latter have so far only yielded Gymnosperms, we may at all events learn something of the primitive forms of these plants, the origin of which is still as problematical as is that of the Angiosperms.

I trust that the criticisms I have made of the theory put forward by Messrs. Arber and Parkin will not be taken as a want of appreciation on my part of the service they have done in formulating a working hypothesis, but merely as an expression of my desire to walk circumspectly in the very alluring paths by which they have sought to explore the primæval forest, and not to emulate those rapid but hazardous flights which have become so fashionable of late.

While the description of new and often intermediate forms of vegetation has aroused such widespread and general interest in Palæobotany, other and more special aspects of the subject have not been without their devotees, and have proved of considerable importance. Morphological anatomy has gained many new points of view, and our knowledge of the evolution of the stele owes much to a careful comparison of recent and fossil forms, even when these investigations have produced conflicting interpretations and divergent views.

Another promising line of Palæobotanical research lies in the direction of investigations of the plant tissues from the physiological and biological points of view. Happily, the vegetable cell-wall is of much greater toughness than that of animal cells, and in consequence the petrified plant-remains found in the calcareous nodules are often so excellently preserved that we can not only study the lignified and corky tissues, but also the more delicate parenchymatous cells. Even root-tips, endosperm, and germinating fern-spores are often so little altered by

¹ Arber, E. A. N., and Parkin, J.: "Studies on the Evolution of Angiosperms," "The Relationship of the Angiosperms to the Gnetales," *Annals of Botany*, vol. xxii, 1908.

² Lignier, O.: "Essai sur l'Evolution morphologique du Règne végétal," *Bulletin de la Soc. Linnaéenne de Normandie*, 6 sér., 3 vol., 1909, réimprimé Février 1911.

fossilisation that their cells can be as easily studied as if the sections had been cut from fresh material. It is this excellence of preservation which has enabled us to gain so complete a knowledge of the anatomy of palæozoic plants, and since the detailed structure of plant organs is often an index of the physical conditions under which the plants grew, we are able to form some opinion as to the habitat of the coal-measure plants. Though a beginning has already been made in this direction by various authors, we have as yet only touched the fringe of the subject, and, as Scott points out in the concluding paragraph of his admirable "Studies," the biology and ecology of fossil plants offer a wide and promising field of research. Such studies are all the more promising, as we now have material from such widely separated localities as the Lancashire coalfield, Westphalia, Moravia, and the Donetz Basin in Russia.

Now that it has been definitely shown by Stopes and Watson that the remains of plants are sometimes continuous through adjacent coal-balls, we may safely accept their conclusion that these calcareous concretions were in the main formed *in situ*, and that the plant-remains they contain represent samples of the vegetable débris of which the coal-seam consists. We have in these petrifications, therefore, an epitome, more or less fragmentary, of the vegetation existing in palæozoic times on the area occupied by the coal-seam, and the Stigmariæ roots in the under-clay, as well as other considerations, lead us to believe that the seam more frequently represents the remains of the coal-measure forest carbonised *in situ*. While this seems to be the more usual formation of coal-seams, it is obvious from the microscopic investigations of coal made by Bertrand, and as has recently been so clearly set forth by Arber in his "Manual on the Natural History of Coal," that in the case of bogheads and cannelles the seam represents metamorphosed sapropelic deposits of lacustrine origin. In other cases, again, considerations of the nature of the coal and the adjacent rocks may incline us to the belief that some, at any rate, of the deposits of coal may be due to material drifted into large lake-basins by river agency.

Broadly speaking, however, and particularly when dealing with the seams from which most of our petrified plant-remains have been collected, we may consider the coal as the accumulated material of palæozoic forests metamorphosed *in situ*. What, then, were the physical and climatic conditions of these primæval forests? The prevalence of wide air-spaces in the cortical tissues of young Calamitean roots, as indeed their earlier name *Myriophylloides* indicates, leads us to believe that, as in the case of many of their existing relatives, they were rooted under water or in waterlogged soil. We gather the same from the structure of Stigmariæ, while the narrow xerophytic character of the leaves at any rate of the tree-like Calamites and Lepidodendra closely resembles the modifications met with in our marsh plants. It has been suggested by several authors that the xerophytic character of the foliage of many of our coal-measure plants may be due to the fact that they inhabited a salt marsh. A closer examination of the foliage, however, of such plants as Lepidodendron and Sigillaria does not reveal the characteristic succulence associated with the foliage of most Halophytes, and in view of the absence of such water-storing parenchyma, the well-developed transmutation-cells of the Lepidodendreae can only be taken to be a xerophytic modification such as is met with in recent Conifers.

The specialisation of the tissues indeed is only such as is quite in keeping with the xerophytic nature of marsh plants. Moreover, the particular group of Equisetales are quite typical of fresh water, and we should expect that if their ancestors had been Halophytes, some at any rate at the present day would have retained this mode of life. Nor have we at the present time any halophytic Lycopodiæ, while Isoëtes, the nearest relative to the Lepidodendra, is an aquatic or sub-aquatic form associated with fresh water.

Among the Filicales, *Acrostichum aureum* seems to be the only halophytic form, inhabiting as it does the swamps of the Ceylon littoral,³ and though, as Miss Thomas has

¹ Tansley, A. G., and Fritsch: "The Flora of the Ceylon Littoral," *New Phytologist*, vol. iv, 1905.

pointed out, its root structure is in close agreement with that of many palæozoic plants, its frond shows considerable deviation from that of *Lyginodendron* or *Medullosa*, both of which plants, as Pteridosperms, are on a higher plane of evolution, and might therefore be expected to show a more highly differentiated type of leaf. But, on the contrary, these coal-measure plants show a more typically Filicinae character, both as regards the finely dissected lamina, and also in the more delicate texture of the foliage compared with the specialised organisation of the frond of *Acrostichum aureum*, described by Miss Thomas.

Nor is it necessary to call to aid the salinity of the marsh to explain the excellent preservation of the tissues of the plant-remains in the so-called coal-balls, in view of the well-known power of humic compounds to retard the decay of vegetable tissues. In addition to these arguments, I might direct attention to the presence of certain fungi among the petrified débris, as more likely to be found in fresh water than in marine conditions. Peronosporites, so common in the decaying Lepidodendroid wood, and the Urophlyctis-like parasite of Stigmarian rootlets, seem to me to support the fresh-water nature of the swamp; just as the occurrence of the mycorhiza, described by Osborn, in the roots of Cordaites seems to indicate the presence of a peaty substratum for the growth of that plant. Potonié also refers to the occasional occurrence of Myriapoda and fresh-water shells as indicative of the fresh-water origin of at least many of the coal-deposits, and a common feature of the petrified remains of coal-measure plants is the occurrence of the excrements of some wood-boring larvæ in the passages tunnelled by these palæozoic organisms through the wood of various stems.

A strong argument in favour of the brackish nature of these swamps would be supplied by the definite identification of Traquairia or Sporocarpion as Radiolaria, though we must remember that certain marine Coelenterata find their way up into the Norfolk Broads, and fresh-water Medusæ are by no means unknown in different parts of the tropics. Of course, if the coal-measure swamps were estuarine or originated in fresh-water lagoons near the sea, they may have been liable from time to time to invasions of salt water, sufficient to account for the presence of occasional marine animals, but without constituting a halophytic plant association.

Potonié, who has made so close a study of the formation of coal, and supports the theory of its fresh-water origin, considered for a long time the comparison between the coal-measure swamp and the cypress swamps of North America, as the nearest but at the same time a somewhat remote analogy, more particularly as he believed that the nature of the coal-measure vegetation required a tropical and also a moister climate than obtains in the southern States of North America. Though, in view of the great development of Pteridophytic vegetation in countries like New Zealand, I think Potonié possibly exaggerates the temperature factor, he is probably right in assuming a fairly warm climate for the coal-measure forest. The difficulty, so far, has been to account for the great thickness of humic or peaty deposits which must have accumulated for the formation of our coal-seams, in view of the fact that extensive peat-formation is generally associated with a low temperature. In the tropics, peat may be deposited at high altitudes, where there is low temperature and high rainfall, but it is generally supposed that the rate of decomposition of vegetable remains is so active that lowland peat-formation was out of the question. Dr. Koorders, however, has observed a peat-producing forest in the extensive plain on the east side of Sumatra, about a hundred miles from the coast. This swamp-forest has been recently re-explored at the instance of Prof. Potonié, and he finds it to agree closely with the vegetative peculiarities which he considers must have been presented by the vegetation of the coal-measure forest. A typical "Sumpflachmoor," this highly interesting tropical swamp has produced a deposit of peat amounting in some places to 30 feet in thickness. The peat itself consists mainly of the remains of the Angiospermic vegetation of which the forest is made up, including pollen-grains and occasional fungal filaments; the preservative power, which has enabled this accumulation of débris to take place, being due to the peaty water which is seen above the roots of the bulk of the vegetation. The

latter consists mainly of dicotyledonous trees belonging to various natural orders, and they mostly show such special adaptations as breathing roots (pneumatophores) and often buttress roots. With the exception of a tree-fern, Pteridophyta, Liverworts, and Mosses, and, indeed, all herbaceous vegetation, are poorly represented in this swamp, though high up in the branches of the trees there is a fair number of epiphytes, and at the edge of the swamp-forest lianes, belonging particularly to the palms, play an important part in the vegetation. The water, partly on account of its peaty nature, partly owing to the intense shade, is almost devoid of Algae, and none of these organisms were found in the peat itself. The interesting account given by Potonié of this tropical peat-formation is very suggestive when certain features, as, for example, the absence or relative paucity of certain of the lower groups of plants, such as Algae and Bryophyta, in the peat, are compared with the plant-remains in some of our coal-seams. Replacing the now dominant Angiosperms by their Pteridophytic representatives in palæozoic times, we have a very close parallel in the two formations.

Another interesting question arises when we consider the great variety of types of vegetation met with among the plant-remains of the coal-seams. For in addition to the limnophilous Calamites and Lepidodendraceæ mentioned above, the coal-balls abound with the remains of representatives of the Filicales, the Pteridospermæ, and the Cordaitaceæ. Were these also members of this swamp vegetation, or have their remains been carried by wind or water from surrounding areas? With regard to some plant-remains, namely, those found exclusively in the roof nodules, the latter was undoubtedly the case; for we have ample evidence, both in their preservation and their mode of occurrence, that they have drifted into the region of the coal-measure swamp after its submergence below the sea. This would apply to such plants as *Tubicaulis Sutcliffii* (Stopes), *Sutcliffia insignis* (Scott), *Cycadoxylon robustum*, and *Poroxyton Sutcliffii*, and other forms, the remains of which have so far not been observed in the coal-seam itself. These plants represent a vegetation of non-aquatic type, and may be taken to have grown on the land areas surrounding the palæozoic swamps. But, on the other hand, we have remains of many non-aquatic plants in the coal-seam itself, closely associated with fragments of typical marsh-plants. How can their juxtaposition be explained?

The advance of our knowledge of ecology points, I think, to a solution of this difficulty. No feature of this fascinating study, which has of late gained so prominent a place in botanical investigation, is more interesting than to trace out the succession of plant associations within the same area, noting the ever-changing conditions which the development of each association brings about. If we follow with Schroeter the gradual development of a lacustrine vegetation from the reed-swamp through the marsh (or Flachmoor) to a peat-moor (Hochmoor), we see how one plant association makes place in its turn for another. May not the mixture of various types of vegetation which we meet with in the petrifications of our coal-seam represent the transition from the open Calamitean or Lepidodendroid swamp to a fen or marsh with plentiful peat-formation, due to the gradual filling up of the stagnant water with plant-remains? Thus in places, at any rate, a transition from aquatic to more terrestrial types of vegetation would take place, while the tree-like forms rooted in the deeper water would continue to flourish. The coal-measure swamp in this stage would differ from the tropical swamp of Koorders by a more abundant undergrowth of herbaceous and climbing plants, rooted in damp humus and passing off gradually into drier peat. Such an undergrowth of Cryptogamic types, mainly Filicinae or Pteridospermic, would have admirable conditions for luxuriant development, apart from the provision of a suitable substratum for its roots, owing to the narrow xerophytic nature of the foliage on the canopy of the trees under which it grew.

Here, too, we see the explanation of the striking difference between the microphyllous and arborescent Calamites and Lepidodendraceæ, and the large ombrophile foliage of the Filicinae and Pteridosperms, which spread out their shade-leaves under the cover of marsh xerophytes, in exactly the same way as Prof. Yapp has so admirably

depicted for recent plants in his account of the "Stratification in the Vegetation of a Marsh."

The development of a mesophytic vegetation in the shelter of the marsh xerophytes makes it unnecessary to postulate an obscuration of the intense sunlight by vapours, as was done by Unger and Saporta for the Carboniferous period. The assumption of a variety of conditions of plant life within the same area helps materially to clear up the difficulties presented by the somewhat incongruous occurrences met with in the petrified plant-remains. The presence of fragments of Cordaites, mixed with those of Calamites and Lepidodendra, in the coal-balls cannot always be explained either by a drift theory or by conceiving the fragments to be wind-borne; but, given an area of retrogressive peat above the ordinary water-level, even so xerophytic a plant as Cordaites might well establish itself there, its mycorhiza-containing roots being well adapted for growth in drier peat. The curious occurrence of more or less concentric rings in the secondary wood of the stem and roots of Cordaites may represent a response, probably not to annual variations of climate, but to abnormal periods of drought, which would affect the upper-peat layers, but not the water-logged soil in which were rooted the Calamites and Lepidodendra.

If, as I suspect, we had in the peat deposit of the coal-seam a succession of associations, we ought to find its growth and history recorded by the sequence of the plant-remains, very much as Mr. Lewis has discovered with such signal success in our Scottish peat-bogs. That some differences occur in the plant-remains building up a seam can be noted by a microscopic examination of the coal itself, in which, as Mr. Lomax tells me, the spores of Lepidodendra occur in definite bands. But no systematic attempt has as yet been made to investigate from this point of view the seams charged with petrified plant débris. Before the Shore pit, which was reopened last summer through the renewed generosity of Mr. Sutcliffe, was finally closed down, I obtained two series of nodules, ranging from the floor to the roof of the seam, and have had these cut for detailed examination. I should not, however, like to make any generalisation from these isolated series, but intend, during the coming winter, to investigate in the same manner further series taken from large blocks of nodules, which have been removed bodily so as to retain the position they occupied in the seam. Though at present the data are only fragmentary, there seems to be some indication that the plant-remains are not without some relation to their position in the seam. Of course, Stigmariaceous rootlets are ubiquitous, and in the nodules of the lower part of the seam predominant, but other plant-remains appear to be more frequently found at one level of the seam than another. The problem, however, is very involved, and it has become apparent that it is as important to study the fine débris in which the larger fragments are embedded as the distribution of these latter. Moreover, attention must be paid to the stage of decomposition presented by the particles forming the matrix of the nodule, as this varies in the lower and upper parts of a seam, very much as in a peat-bed we can distinguish the lighter-coloured fibrous peat from the darker layers at the base of a peat-cutting. Mr. Lomax, who has a unique experience of these coal-balls, informs me that he can tell whether a nodule is from the top or bottom of the seam by the lighter or darker colour of the matrix. The importance of applying the methods which have been so successful in elucidating the history of modern peat-deposits to the investigation of the coal-seam will be clearly appreciated both by palaeobotanists and ecologists, and this particular problem offers a striking illustration of the interdependence of various branches of botanical investigation. It is fortunate, indeed, that the two fields of work, Palaeobotany and Plant Ecology, though they have been subjected to fairly intensive cultivation, have not become exclusively the domain of specialists. The strength and progress of modern Botany have been due to the close collaboration of workers engaged in different branches of botanical science, and the fact that British ecologists have combined to attack a series of the problems from very diverse points of view leads one to hope that, with a continuance of that intimate cooperation which has characterised their work so far, and with the added stimulus of the friendly

visit of our distinguished colleagues from abroad, considerable progress may be expected in the future in this branch of botanical study. Privileged as I have been to assist at the deliberations of the British ecologists, without as yet having taken any active part in their work, I feel myself at liberty to point with appreciation to the excellent beginning they have made of a botanical survey of Great Britain and Ireland, as well as to the more detailed investigations of special associations and formations, such as the woodlands, the moorlands, the fens, the broads, salt marshes, and shingle beaches. I am glad to think that our foreign visitors have been able to see these interesting types of vegetation under the guidance of those who have made a special study of these subjects.

The importance to ecologists of an up-to-date critical Flora was dwelt upon by my predecessor in this presidential chair, and this obvious need may be regarded as a further illustration of the inter-relationship of the various aspects of Botanical Science. Though it has been obvious to all that the swing of the pendulum has been for a long time away from pure systematic botany, I am convinced that the great development of plant ecology, of which we have many indications, will not merely lessen the momentum of the swinging pendulum, but will draw the latter back towards a renewed and critical study of the British flora. That a revival of interest in systematic botany will come through the labours of those who are engaged in survey work and other forms of ecological study, is foreshadowed by the fact that Dr. Moss has undertaken to edit a "New British Flora," which will, I believe, largely fulfil the objects put forward by Prof. Trail in his Presidential Address. I trust, however, that in addition to the ecologists, those botanists who are interested in genetics will contribute their share towards the completion of our knowledge of critical species, varieties, and hybrids, all of which offer such intricate problems alike to the systematist and to the student of genetics.

De Vries prefaced his lectures on "Species and Varieties, their Origin by Mutation," with the pregnant sentence: "The origin of species is an object of experimental investigation," and this is equally true of the study of the real and presumptive hybrids of our British flora, which may be investigated either synthetically or, when fertile, also analytically, as in some cases their offspring show striking Mendelian segregation. Some good work has already been accomplished in this direction, but more remains to be done, and we have here an important and useful sphere of work for the energies of many skilled plant-breeders.

I would therefore like to plead for intimate collaboration between all botanists, hopeful that, as progress in the past has come through the labours of men of wide sympathies, so in the future, when studies are bound to become more specialised, there will be no narrowing of interests, but that the various problems which have to be solved will be attacked from all points of view, the morphological, the physiological, the ecological, and the systematic. Thus by united efforts and close cooperation of botanists of all schools and of all countries we shall gain the power to surmount the difficulties with which our science is still confronted.

SUB-SECTION K.

AGRICULTURE.

OPENING ADDRESS BY W. BATESON, M.A., F.R.S.,
CHAIRMAN OF THE SUB-SECTION.

The invitation to preside over the Agricultural Sub-section on this occasion naturally gave me great pleasure, but after accepting it I have felt embarrassment in a considerable degree. The motto of the great Society which has been responsible for so much progress in agricultural affairs in this country very clearly expresses the subject of our deliberations in the words "Practice with Science," and to be competent to address you, a man should be well conversant with both. But even if agriculture is allowed to include horticulture, as may perhaps be generally conceded, I am sadly conscious that my special qualifications are much weaker than you have a right to demand of a President.

The aspects of agriculture from which it offers hopeful

lines for scientific attack are, in the main, three: Physiological, Pathological, and Genetic. All are closely inter-related, and for successful dealing with the problems of any one of these departments of research, knowledge of the results attained in the others is now almost indispensable. I myself can claim personal acquaintance with the third or genetic group alone, and therefore in considering how science is to be applied to the practical operations of agriculture, I must necessarily choose it as the more special subject of this address. I know very well that wider experience of those other branches of agricultural science or practical agriculture would give to my remarks a weight to which they cannot now pretend.

Before, however, proceeding to these topics of special consideration, I have thought it not unfitting to say something of a more general nature as to the scope of an applied science, such as that to which we here are devoted. We are witnessing a very remarkable outburst of activity in the promotion of science in its application to agriculture. Public bodies distributed throughout this country and our possessions are organising various enterprises with that object. Agricultural research is now everywhere admitted as a proper subject for University support and direction.

With the institution of the Development Grant a national subsidy is provided on a considerable scale in England for the first time.

At such a moment the scope of this applied science and the conditions under which it may most successfully be advanced are prominent matters of consideration in the minds of most of us. We hope great things from these new ventures. We are, however, by no means the first to embark upon them. Many of the other great nations have already made enormous efforts in the same direction. We have their experience for a guide.

Now, it is not in dispute that wherever agricultural science has been properly organised valuable results have been attained, some of very high importance indeed; yet with full appreciation of these achievements, it is possible to ask whether the whole outcome might not have been greater still. In the course of recent years I have come a good deal into contact with those who in various countries are taking part in such work, and I have been struck with the unanimity that they have shown in their comments on the conditions imposed upon them. Those who receive large numbers of agricultural bulletins purporting to give the results of practical trials and researches will, I feel sure, agree with me that with certain notable exceptions they form on the whole dull reading. True they are in many cases written for farmers and growers in special districts, rather than for the general scientific reader, but I have sometimes asked myself whether those farmers get much more out of this literature than I do. I doubt it greatly. Nevertheless, to the production of these things much labour and expense have been devoted. I am sure, and I believe that most of those engaged in these productions themselves feel, that the effort might have been much better applied elsewhere. Work of this unnecessary kind is done, of course, to satisfy a public opinion which is supposed to demand rapid returns for outlay, and to prefer immediate apparent results, however trivial, to the long delay which is the almost inevitable accompaniment of any serious production. For my own part, I much doubt whether in this estimate present public opinion has been rightly gauged. Enlightenment as to the objects, methods, and conditions of scientific research is proceeding at a rapid rate. I am quite sure, for example, that no organisation of agricultural research now to be inaugurated under the Development Commission will be subjected to the conditions laid down in 1887 when the Experimental Stations of the United States were established. For them it is decreed in Sect. 4 of the Act of Establishment:—

"That bulletins or reports of progress shall be published at said stations at least once in three months, one copy of which shall be sent to each newspaper in the States or Territories in which they are respectively located, and to such individuals actually engaged in farming as may request the same and as far as the means of the station will permit."

It would be difficult to draft a condition more unfavourable to the primary purpose of the Act, which was "to conduct original researches or verify experiments on the

physiology of plants and animals" with agricultural objects in view. I can scarcely suppose the most prolific discoverer should be invited to deliver himself more than once a year. Not only does such a rule compel premature publication—that nuisance of modern scientific life—but it puts the investigator into a wrong attitude towards his work. He will do best if he forget the public and the newspaper of his State or Territory for long periods, and should only return to them when, after repeated verification, he is quite certain he has something to report.

In this I am sure the best scientific opinion of all countries would be agreed. If it is true that the public really demand continual scraps of results, and cannot trust the investigators to pursue research in a reasonable way, then the public should be plainly given to understand that the time for inaugurating researches in the public's name has not arrived. Men of science have in some degree themselves to blame if the outer world has been in any mistake on these points. It cannot be too widely known that in all sciences, whether pure or applied, research is nearly always a very slow process, uncertain in production and full of disappointments. This is true, even in the new industries, chemical and electrical, for instance, where the whole industry has been built up from the beginning on a basis developed entirely by scientific method and by the accumulation of precise knowledge. Much more must any material advance be slow in the case of an ancient art like agriculture, where practice represents the casual experience of untold ages and accurate investigation is of yesterday. Problems, moreover, relating to unorganised matter are in their nature simpler than those concerned with the properties of living things, a region in which accurate knowledge is more difficult to attain. Here the research of the present day can aspire no higher than to lay the foundation on which the following generations will build. When this is realised it will at once be perceived that both those who are engaged in agricultural research and those who are charged with the supervision and control of these researches must be prepared to exercise a large measure of patience.

The applicable science must be created before it can be applied. It is with the discovery and development of such science that agricultural research will for long enough best occupy its energies. Sometimes, truly, there come moments when a series of obvious improvements in practice can at once be introduced, but this happens only when the penetrative genius of a Pasteur or a Mendel has worked out the way into a new region of knowledge, and returns with a treasure that all can use. Given the knowledge it will soon enough become applied.

I am not advocating work in the clouds. In all that is attempted we must stick near to the facts. Though the methods of research and of thought must be strict and academic, it is in the farm and the garden that they must be applied. If inspiration is to be found anywhere it will be there. The investigator will do well to work

"As if his highest plot
To plant the bergamot."

It is only in the closest familiarity with phenomena that we can attain to that perception of their orderly relations, which is the beginning of discovery.

To the creation of applicable science the very highest gifts and training are well devoted. In a foreign country an eminent man of science was speaking to me of a common friend, and he said that as our friend's qualifications were not of the first rank he would have to join the agricultural side of the university. I have heard remarks of similar disparagement at home. Now, whether from the point of view of agriculture or pure science, I can imagine no policy more stupid and short-sighted.

The man who devotes his life to applied science should be made to feel that he is in the main stream of scientific progress. If he is not, both his work and science at large will suffer. The opportunities of discovery are so few that we cannot afford to miss any, and it is to the man of trained mind who is in contact with the phenomena of a great applied science that such opportunities are most often given. Through his hands pass precious material, the outcome sometimes of years of effort and design. To tell

him that he must not pursue that inquiry further because he cannot foresee a direct and immediate application of the knowledge is, I believe almost always, a course detrimental to the real interests of the applied science. I could name specific instances where in other countries thoroughly competent and zealous investigators have by the shortsightedness of superior officials been thus debarred from following to their conclusion researches of great value and novelty.

In this country, where the Development Commission will presumably for many years be the main instigator and controller of agricultural research, the constitution of the Advisory Board, on which Science is largely represented, forms a guarantee that broader counsels will prevail, and it is to be hoped that not merely this inception of the work, but its future administration also, will be guided in the same spirit. So long as a train of inquiry continues to extend, and new knowledge, that most precious commodity, is coming in, the enterprise will not be in vain and it will be usually worth while to pursue it.

The relative value of the different parts of knowledge in their application to industry is almost impossible to estimate, and a line of work should not be abandoned until it leads to a dead end, or is lost in a desert of detail.

We have, not only abroad, but also happily in this country, several private firms engaged in various industries—I may mention especially metallurgy, pharmacy, and brewing—who have set an admirable example in this matter, instituting researches of a costly and elaborate nature, practically unlimited in scope, connected with the subjects of their several activities, conscious that it is only by men in close touch with the operations of the industry that the discoveries can be made, and well assured that they themselves will not go unrewarded.

Let us on our part beware of giving false hopes. We know no haemony "of sovran use against all enchantments, mildew blast, or damp." Those who are wise among us do not even seek it yet. Why should we not take the farmer and gardener into our fullest confidence and tell them this? I read lately a newspaper interview with a fruit-farmer who was being questioned as to the success of his undertaking, and spoke of the pests and difficulties with which he had had to contend. He was asked whether the Board of Agriculture and the scientific authorities were not able to help him. He replied that they had done what they could, that they had recommended first one thing and then another, and he had formed the opinion that they were only in an experimental stage. He was perfectly right, and he would hardly have been wrong had he said that in these things science is only approaching the experimental stage. This should be notorious. There is nothing to extenuate. To affect otherwise would be unworthy of the dignity of science.

Those who have the means of informing the public mind on the state of agricultural science should make clear that though something can be done to help the practical man already, the chief realisation of the hopes of that science is still very far away, and that it can only be reached by long and strenuous effort, expended in many various directions, most of which must seem to the uninitiated mere profitless wandering. So only will the confidence of the laity be permanently assured towards research.

Nowhere is the need for wide views of our problems more evident than in the study of plant-diseases. Hitherto this side of agriculture and of horticulture, though full of possibilities for the introduction of scientific method, has been examined only in the crudest and most empirical fashion. To name the disease, to burn the affected plants, and to ply the crop with all the sprays and washes in succession ought not to be regarded as the utmost that science can attempt. There is at the present time hardly any comprehensive study of the morbid physiology of plants comparable with that which has been so greatly developed in application to animals. The nature of the resistance to disease characteristic of so many varieties, and the modes by which it may be ensured, offers a most attractive field for research, but it is one in which the advance must be made by the development of pure science, and those who engage in it must be prepared for a long period of labour without ostensible practical results. It has seemed to me that the most likely method of attack is

here, as often, an indirect one. We should probably do best if we left the direct and special needs of agriculture for a time out of account, and enlisted the services of pathologists trained in the study of disease as it affects man and animals, a science already developed and far advanced towards success. Such a man, if he were to devote himself to the investigation of the same problems in the case of plants, could, I am convinced, make discoveries which would not merely advance the theory of disease-resistance in general very greatly, but would much promote the invention of rational and successful treatment.

As regards the application of Genetics to practice, the case is not very different. When I go to the Temple Show or to a great exhibition of live stock, my first feeling is one of admiration and deep humility. Where all is so splendidly done and results so imposing are already attained, is it not mere impertinence to suppose that any advice we are able to give is likely to be of value?

But so soon as one enters into conversation with breeders, one finds that almost all have before them some ideal to which they have not yet attained, operations to perform that they would fain do with greater ease and certainty, and that, as a matter of fact, they are looking to scientific research as a possible source of the greater knowledge which they require. Can we, without presumption, declare that genetic science is now able to assist these inquirers? In certain selected cases it undoubtedly can—and I will say, moreover, that if the practical men and we students could combine our respective experiences into one head, these cases would already be numerous. On the other hand, it is equally clear that in a great range of examples practice is so far ahead that science can scarcely hope in finite time even to represent what has been done, still less to better the performance. We cannot hope to improve the Southdown sheep for its own districts, to take a second off the trotting record, to increase the flavour of the muscat of Alexandria, or to excel the orange and pink of the rose Juliet. Nothing that we know could have made it easier to produce the Rambler roses, or even to evoke the latest novelties in sweet peas, though it may be claimed that the genetic system of the sweet pea is, as things go, fairly well understood. To do any of these things would require a control of events so lawless and rare that for ages they must probably remain classed as accidents. On the other hand, the modes by which combinations can be made, and by which new forms can be fixed, are through Mendelian analysis and the recent developments of genetic science now reasonably clear, and with that knowledge much of the breeder's work is greatly simplified. This part of the subject is so well understood that I need scarcely do more than allude to it.

A simple and interesting example is furnished by the work which Mr. H. M. Leake is carrying out in the case of cotton in India. The cottons of fine quality grown in India are monopodial in habit, and are consequently late in flowering. In the United Provinces a comparatively early-flowering form is required, as otherwise there is not time for the fruits to ripen. The early varieties are sympodial in habit, and the primary apex does not become a flower. Hitherto no sympodial form with cotton of high quality has existed, but Mr. Leake has now made the combination needed, and has fixed a variety with high-class cotton and the sympodial habit, which is suitable for cultivation in the United Provinces. Until genetic physiology was developed by Mendelian analysis, it is safe to say that a practical achievement of this kind could not have been made with rapidity or certainty. The research was planned on broad lines. In the course of it much light was obtained on the genetics of cotton, and features of interest were discovered which considerably advance our knowledge of heredity in several important respects. This work forms an admirable illustration of that simultaneous progress both towards the solution of a complex physiological problem and also towards the successful attainment of an economic object which should be the constant aim of agricultural research.

Necessarily it follows that such assistance as genetics can at present give is applicable more to the case of plants and animals which can be treated as annuals than to creatures of slower generation. Yet this already is a large area of operations. One of the greatest advances to be claimed for the work is that it should induce raisers of seed crops

especially to take more hopeful views of their absolute purification than have hitherto prevailed. It is at present accepted as part of the natural perversity of things that most high-class seed crops must throw "rogues," or that at the best the elimination of these waste plants can only be attained by great labour extended over a vast period of time. Conceivably that view is correct, but no one acquainted with modern genetic science can believe it without most cogent proof. Far more probably we should regard these rogues either as the product of a few definite individuals in the crop, or even as chance impurities brought in by accidental mixture. In either case they can presumably be got rid of. I may even go further and express a doubt whether that degeneration which is vaguely supposed to be attendant on all seed crops is a physiological reality. Degeneration may perhaps affect plants like the potato which are continually multiplied asexually, though the fact has never been proved satisfactorily. Moreover, it is not in question that races of plants taken into unsuitable climates do degenerate rapidly from uncertain causes, but that is quite another matter.

The first question is to determine whether a given rogue has in it any factor which is dominant to the corresponding character in the typical plants of the crop. If it has, then we may feel considerable confidence that these rogues have been introduced by accidental mixture. The only alternative, indeed, is cross-fertilisation with some distinct variety possessing the dominant, or crossing within the limits of the typical plants themselves occurring in such a way that complementary factors have been brought together. This last is a comparatively infrequent phenomenon, and need not be considered till more probable hypotheses have been disposed of. If the rogues are first crosses the fact can be immediately proved by sowing their seeds, for segregation will then be evident. For example, a truly round seed is occasionally, though very rarely, found on varieties of pea which have wrinkled seeds. I have three times seen such seeds on my own plants. A few more were kindly given me by Mr. Arthur Sutton, and I have also received a few from M. Philippe de Vilmorin—to both of whom I am indebted for most helpful assistance and advice. Of these abnormal or unexpected seeds some died without germinating, but all which did germinate in due course produced the normal mixture of round and wrinkled, proving that a cross had occurred. Cross-fertilisation in culinary peas is excessively rare, but it is certainly sometimes effected, doubtless by the leaf-cutter bee (*Megachile*) or a humble-bee visiting flowers in which for some reason the pollen has been inoperative. But in peas crossing is assuredly not the source of the ordinary rogues. These plants have a very peculiar conformation, being tall and straggling, with long internodes, small leaves, and small flowers, which together give them a curious wild look. When one compares them with the typical cultivated plants which have a more luxuriant habit, it seems difficult to suppose that the rogue can really be recessive to such a type. True, we cannot say definitely *a priori* that any one character is dominant to another, but old preconceptions are so strong that without actual evidence we always incline to think of the wilder and more primitive characteristics as dominants. Nevertheless, from such observations as I have been able to make, I cannot find any valid reason for doubting that the rogues are really recessives to the type. One feature in particular is quite inconsistent with the belief that these rogues are in any proper sense degenerative returns to a wild type, for in several examples the rogues have pointed pods like the cultivated sorts from which they have presumably been derived. All the more primitive kinds have the dominant stump-ended pod. If the rogues had the stump pods they would fall into the class of dominants, but they have no single quality which can be declared to be certainly dominant to the type, and I see no reason why they may not be actually recessives to it after all. Whether this is the true account or not we shall know for certain next year. Mr. Sutton has given me a quantity of material which we are now investigating at the John Innes Horticultural Institution, and by sowing the seed of a great number of individual plants separately I anticipate that we shall prove the rogue-throwers to be a class apart. The pure types then separately saved should, according to expectation, remain rogue-free, unless further sporting or

fresh contamination occurs. If it prove that the long and attenuated rogues are really recessive to the shorter and more robust type, the case will be one of much physiological significance, but I believe a parallel already exists in the case of wheats, for among certain crosses bred by Prof. Biffen, some curious spelt-like plants occurred among the derivatives from such robust wheats as Rivet and Red Fife.

There is another large and important class of cases to which similar considerations apply. I refer to the bolting or running to seed of crops grown as biennials, especially root crops. It has hitherto been universally supposed that the loss due to this cause, amounting in Sugar Beet as it frequently does to five, or even more, per cent., is not preventable. This may prove to be the truth, but I think it is not impossible that the bolters can be wholly, or almost wholly, eliminated by the application of proper breeding methods. In this particular example I know that season and conditions of cultivation count for a good deal in promoting or checking the tendency to run to seed; nevertheless one can scarcely witness the sharp distinction between the annual and biennial forms without suspecting that genetic composition is largely responsible. If it proves to be so, we shall have another remarkable illustration of the direct applicability of knowledge gained from a purely academic source. "Let not him that girdeth on his harness boast himself as he that putteth it off," and I am quite alive to the many obstacles which may lie between the conception of an idea and its realisation. One thing, however, is certain, that we have now the power to formulate rightly the question which the breeder is to put to nature; and this power and the whole apparatus by which he can obtain an answer to his question—in whatever sense that answer may be given—has been derived from experiments designed with the immediate object of investigating that scholastic and seemingly barren problem, "What is a species?" If Mendel's eight years' work had been done in an agricultural school supported by public money, I can imagine much shaking of heads on the County Council governing that institution, and yet it is no longer in dispute that he provided the one bit of solid discovery upon which all breeding practice will henceforth be based.

Everywhere the same need for accurate knowledge is apparent. I suppose horse-breeding is an art which has by the application of common sense and great experience been carried to about as high a point of perfection as any. Yet even here I have seen a mistake made which is obvious to anyone accustomed to analytical breeding. Among a number of stallions provided at great expense to improve the breed of horses in a certain district was one which was shown me as something of a curiosity. This particular animal had been bred by one of the provided stallions out of an indifferent country mare. It had been kept as an unusually good-looking colt, and was now travelling the country as a breeding stallion, under the highest auspices. I thought to myself that if such a practice is sanctioned by breeding acumen and common sense, Science is not, after all, so very ambitious if she aspires to do rather better. The breeder has continually to remind himself that it is not what the animal or plant looks that matters, but what it is. Analysis has taught us to realise, first, that each animal and plant is a double structure, and next that the appearance may show only half its composition.

With respect to the inheritance of many physiological qualities of divers kinds we have made at least a beginning of knowledge, but there is one class of phenomena as yet almost untouched. This is the miscellaneous group of attributes which are usually measured in terms of size, fertility, yield, and the like. This group of characters has more than common significance to the practical man. Analysis of them can nevertheless only become possible when pure science has progressed far beyond the point yet reached.

I know few lines of pure research more attractive and at the same time more likely to lead to economic results than an investigation of the nature of variation in size of the whole organism or of its parts. By what factors is it caused? By what steps does it proceed? By what limitations is it beset? In illustration of the application of these questions I may refer to a variety of topics that have been lately brought to my notice. In the case of merino sheep

I have been asked by an Australian breeder whether it is possible to combine the optimum length of wool with the optimum fineness and the right degree of crimping. I have to reply that absolutely nothing is yet known for certain as to the physiological factors determining the length or the fineness of wool. The crimping of the fibres is an expression of the fact that each particular hair is curved, and if free and untwisted would form a corkscrew spiral, but as to the genetics of curly hair even in man very little is yet known. But leaving the question of curl on one side, we have, in regard to the length and fineness of wool, a problem which genetic experiment ought to be able to solve. Note that in it, as in almost all problems of the "yield" of any product of farm or garden, two distinct elements are concerned—the one is *size*, and the other is *number*. The length of the hair is determined by the rate of excretion and length of the period of activity of the hair follicles, but the fineness is determined by the number of follicles in unit area. Now analogy is never a safe guide, but I think if we had before us the results of really critical experiments on the genetics of size and number of multiple organs in any animal or even any plant, we might not wholly be at a loss in dealing with this important problem.

A somewhat similar question comes from South Africa. Is it possible to combine the qualities of a strain of ostriches which has extra long plumes with those of another strain which has its plumes extra lustrous? I have not been able fully to satisfy myself upon what the lustre depends, but I incline to think it is an expression of fineness of fibre, which again is probably a consequence of the smallness and increased number of the excreting cells, somewhat as the fineness of wool is a consequence of the increased number and smallness of the excreting follicles.

Again the question arises in regard to flax, how should a strain be bred which shall combine the maximum length with maximum fineness of fibre? The element of number comes in here, not merely with regard to the number of fibres in a stem, but also in two other considerations: first, that the plant should not tiller at the base, and, secondly, that the decussation of the flowering branches should be postponed to the highest possible level.

Now in this problem of the flax, and not impossibly in the others I have named, we have questions which can in all likelihood be solved in a form which will be of general, if not of universal, application to a host of other cognate questions. By good luck the required type of flax may be struck at once, in which case it may be fixed by ordinary Mendelian analysis, but if the problem is investigated by accurate methods on a large scale, the results may show the way into some of those general problems of size and number which make a great part of the fundamental mystery of growth.

I see no reason why these things should remain inscrutable. There is indeed a little light already. We are well acquainted with a few examples in which the genetic behaviour of these properties is fairly definite. We have examples in which, when two varieties differing in number of divisions are crossed, the lower number dominates—or, in other words, that the increased number is a consequence of the removal of a factor which prevents or inhibits particular divisions, so that they do not take place. It is likely that in so far as the increased productivity of a domesticated form as compared with its wild original depends on more frequent division, the increase is due to loss of inhibiting factors. How far may this reasoning be extended? Again, we know that in several plants—peas, sweet peas, *Antirrhinum*, and certain wheats—a tall variety differs in that respect from a dwarf in possessing one more factor. It would be an extraordinarily valuable addition to knowledge if we could ascertain exactly how this factor operates, how much of its action is due to linear repetition, and how much to actual extension of individual parts. The analysis of the plants of intermediate size has never been properly attempted, but would be full of interest and have innumerable bearings on other cases in animals and plants, some of much economic importance.

That in all such examples the objective phenomena we see are primarily the consequence of the interaction of genetic factors is almost certain. The lay mind is at first disposed as always, to attribute such distinctions to any-

thing rather than to a specific cause which is invisible. An appeal to differences in conditions—which a moment's reflection shows to be either imaginary or altogether independent—or to those vague influences invoked under the name of Selection, silently postponing any laborious analysis of the nature of the material selected, repels curiosity for a time, and is lifted as a veil before the actual phenomena; and so even critical intelligences may for an indefinite time be satisfied that there is no specific problem to be investigated, in the same facile way that, till a few years ago, we were all content with the belief that malarial fevers could be referred to any damp exhalations in the atmosphere, or that in suppuration the body was discharging its natural humours. In the economics of breeding, a thousand such phenomena are similarly waiting for analysis and reference to their specific causes. What, for instance, is self-sterility? The phenomenon is very widely spread among plants, and is far commoner than most people suppose who have not specially looked for it. Why is it that the pollen of an individual in these plants fails to fertilise the ova of the same individual? Asexual multiplication seems in no way to affect the case. The American experimenters are doubtless right in attributing the failure of large plantations of a single variety of apples or of pears in a high degree to this cause. Sometimes, as Mr. W. O. Backhouse has found in his work on plums at the John Innes Horticultural Institution, the behaviour of the varieties is most definite and specific. He carefully self-fertilised a number of varieties, excluding casual pollination, and found that while some sorts—for example, Victoria, Czar, and Early Transparent—set practically every fruit self-pollinated, others, including several (perhaps all) Greengages, Early Orleans, and Sultan, do not set a single fruit without pollination from some other variety. Dr. Erwin Baur has found indications that self-sterility in *Antirrhinum* may be a Mendelian recessive, but whether this important suggestion be confirmed or not, the subject is worth the most minute study in all its bearings. The treatment of this problem well illustrates the proper scope of an applied science. The economic value of an exact determination of the empirical facts is obvious, but it should be the ambition of anyone engaging in such a research to penetrate further. If we can grasp the *rationale* of self-sterility we open a new chapter in the study of life. It may contain the solution of the question What is an individual?—no mere metaphysical conundrum, but a physiological problem of fundamental significance.

What, again, is the meaning of that wonderful increase in size or in "yield" which so often follows on a first cross? We are no longer content, as Victorian teleology was, to call it a "beneficial" effect and pass on. The fact has long been known and made use of in breeding stock for the meat market, and of late years the practice has also been introduced in raising table poultry. Mr. G. N. Collins,¹ of the U.S. Department of Agriculture, has recently proposed with much reason that it might be applied in the case of maize. The cross is easy to make on a commercial scale, and the gain in yield is striking, the increase ranging as high as 95 per cent. These figures sound extravagant, but from what I have frequently seen in peas and sweet peas, I am prepared for even greater increase. But what is this increase? How much of it is due to change in number of parts, how much to transference of differentiation or homeosis, as I have called it—leaf-buds becoming flower-buds, for instance—and how much to actual increase in size of parts? To answer these questions would be to make an addition to human knowledge of incalculably great significance.

Then we have the further question, How and why does the increase disappear in subsequent generations? The very uniformity of the cross-breds between pure strains must be taken as an indication that the phenomenon is orderly. Its subsidence is probably orderly also. Shull has advocated the most natural view that heterozygosis is the exciting cause, and that with the gradual return to the homozygous state the effects pass off. I quite think this may be a part of the explanation, but I feel difficulties, which need not here be detailed, in accepting this as a complete account. Some of the effect we may probably also attribute to the combination of complementary factors;

¹ Bureau of Plant Industry, Bulletin No. 191, 1910.

but whether heterozygosis, or complementary action, is at work, our experience of cross-breeding in general makes it practically certain that genetic factors of special classes only can have these properties, and no pains should be spared in identifying them. It is not impossible that such identification would throw light on the nature of cell division and of that meristic process by which the repeated organs of living things are constituted, and I have much confidence that in the course of the analysis discoveries will be made bearing directly both on the general theory of heredity and on the practical industry of breeding.

In the application of science to the arts of agriculture, chemistry, the foundation of sciences, very properly and inevitably came first, while breeding remained under the unchallenged control of simple common sense alone. The science of genetics is so young that when we speak of what it also can do we must still for the most part ask for a long credit; but I think that if there is full cooperation between the practical breeder and the scientific experimenter, we shall be able to redeem our bonds at no remotely distant date. In the mysterious properties of the living bodies of plants and animals there is an engine capable of wonders scarcely yet suspected, waiting only for the constructive government of the human mind. Even in the seemingly rigorous tests and trials which have been applied to living material apparently homogeneous, it is not doubtful that error has often come in by reason of the individual genetic heterogeneity of the plants and animals chosen. A batch of fruit trees may be all of the same variety, but the stocks on which the variety was grafted have hitherto been almost always seminally distinct individuals, each with its own powers of luxuriance or restriction, their own root-systems, and properties so diverse that only in experiments on a colossal scale can this diversity be supposed to be levelled down. Even in a closely bred strain of cattle, though all may agree in their "points," there may still be great genetic diversity in powers of assimilation and rapidity of attaining maturity, by which irregularities by no means negligible are introduced. The range of powers which organic variation and genetic composition can confer is so vast as to override great dissimilarities in the conditions of cultivation. This truth is familiar to every raiser and grower, who knows it in the form that the first necessity is for him to get the right breed and the right variety for his work. If he has a wheat of poor yield, no amount of attention to cultivation or manuring will give him a good crop. An animal that is a bad doer will remain so in the finest pasture. All praise and gratitude to the student of the conditions of life, for he can do, and has done, much for agriculture, but the breeder can do even more.

When more than fifteen years ago the proposal to found a school of agriculture in Cambridge was being debated, much was said of the importance of the chemistry of soils, of researches into the physiological value of foodstuffs, and of other matters then already prominent on the scientific horizon. I remember then interpolating with an appeal for some study of the physiology of breeding, which I urged should find a place in the curriculum, and I pointed out that the improvement in the strains of plants and animals had done at least as much—more, I really meant—to advance agriculture than had been accomplished by other means. My advice found little favour, and I was taken to task afterwards by a prominent advocate of the new school for raising a side issue. Breeding was a purely empirical affair. Common sense and selection comprised the whole business, and physiology flew at higher game. I am, nevertheless, happy now to reflect that of the work which is making the Cambridge School of Agriculture a force for progress in the agricultural world the remarkable researches and results of my former colleague, Prof. Biffen, based as they have been on modern discoveries in the pure sciences of breeding, occupy a high and greatly honoured place.

In conclusion, I would sound once more the note with which I began. If we are to progress fast there must be no separation made between pure and applied science. The practical man with his wide knowledge of specific natural facts, and the scientific student ever seeking to find the hard general truths which the diversity of Nature hides—truths out of which any lasting structure of progress must be built—have everything to gain from free inter-

NO. 2186, VOL. 87]

change of experience and ideas. To ensure this community of purpose those who are engaged in scientific work should continually strive to make their aims and methods known at large, neither exaggerating their confidence nor concealing their misgivings,

"Till the world is wrought
To sympathy with hopes and tears it heeded not."

UNIVERSITY AND EDUCATIONAL INTELLIGENCE.

BIRMINGHAM.—Dr. Alex. Findlay, special lecturer in physical chemistry, is resigning his post in consequence of his acceptance of the chair of chemistry in the University of Wales at Aberystwyth.

Dr. Murray has resigned his post as assistant lecturer and demonstrator in chemistry, having been appointed head of the chemical and metallurgical department of Wolverhampton Technical School.

By the will of Dr. S. J. Gee, late physician to St. Bartholomew's Hospital, the sum of about 20,000*l.* is left to his daughter for life, with contingent remainder to the Royal College of Physicians, London, upon trust, so far as possible, to form a permanent endowment fund for the college.

THE winter session of the London (Royal Free Hospital) School of Medicine for Women will be opened on Monday, October 2, with an introductory address by Sir Henry Burlin, P.R.C.S., upon "Research in Medicine and Women in Medical Research." Mrs. Garrett Anderson, president of the school, will occupy the chair.

MR. T. HARRIS, of the Imperial College of Science and Technology, and the Cavendish Laboratory, Cambridge, has been appointed lecturer and demonstrator in the physical department of the East London College in succession to Mr. E. Marsden. Mr. P. Kemp has been appointed lecturer in the electrical engineering department of the same college.

THE exchange of professors between Harvard University and the Ministry of Public Instruction in France comes into effect this winter for the first time, and the Bulletin of the American Geographical Society announces that Prof. W. M. Davis will go to Paris to lecture until the end of March, after the International Congress at Rome has ended. Prof. Diehl, of the Sorbonne, will go to Harvard University to lecture on Byzantine history.

A SPECIAL course of twelve lectures on illumination is to be given at the Polytechnic, Regent Street, during the present session. The lectures, which will be under the supervision of Mr. L. Gaster, editor of *The Illuminating Engineer*, will deal with all illuminants, including recent advances in electric, gas, oil, and acetylene lighting; the effect of light on the eye; the hygienic aspects of illumination; and the measurement of light and illumination. Practical problems, such as the lighting of schools, streets, and factories, will be treated in the second half of the course, commencing in January, 1912. Until December 5 the lectures will be held on Tuesday evenings at 7.30, and during January and February next on Thursday evenings at the same hour.

THE new session in the faculties of arts, laws, science, engineering, and medical sciences at University College, London, will begin on October 2. The list of public introductory lectures at the college contains the following, among others:—Wednesday, October 4, Prof. H. R. Kenwood on "The Scope of School Hygiene and the Legislative Provisions dealing with the School Child," being the first of a course of lectures on school hygiene specially designed for school teachers; Friday, October 6, Prof. G. Dawes Hicks on "Bergson's Conception of Creative Evolution" (this lecture is designed as an introduction to a course of four public university lectures to be delivered by Prof. Henri Bergson at University College on October 20, 21, 27, and 28). A course of public lectures on heating and ventilating engineering will be given by Mr. A. H. Barker, the introductory lecture on Tuesday, October 17, being on "Problems in Heating and Ventilation awaiting Solution by the Engineer." On the same day Mr. E.